LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Radiometer and its Lessons

I AM sorry to have again to correct Mr. Stoney; but I cannot allow the statements contained in his letter to pass unnoticed.

1. There is nothing in my earlier paper that is "*admittedly erroneous.*" If there is error in these papers I am not aware of it.

2. These papers do not "conclude with Prof. Reynolds's own expression of opinion that residual gas is not the cause of the force observed by Mr. Crookes." Nor have I ever held or anywhere expressed such an opinion.

3. In the passage to which Mr. Stoney refers, Clausius does not imply that the law established by himself and Maxwell, viz., that the only condition of thermal equilibrium in a gas is that of uniform temperature, depends on the mean path of the molecules; and it was this law that I instanced as being at variance with Mr. Stoney's assumptions (1) that gas is a perfect non-conductor of heat; (2) that a layer of gas across which the temperature varies can exist in a state of thermal equilibrium without the passage of heat from the hotter to the colder part. Mr. Stoney has nowhere that I can see given any proof of these assumptions, and I venture to prefer the authority of Professors Maxwell and Clausius, supported as it is by the whole evidence of facts.

4. Mr. Stoney says that I have excluded the polarisation of gas from my explanation. Mr. Stoney has not, that I am aware, defined what he means by polarisation, but if he measures the polarisation of a gas conducting heat by the excess of momentum carried across any ideal surface in one direction over and above that which is carried in the opposite, this polarisation is independent of the length of the mean path, and forms an essential part of my explanation.

There is one statement in Mr. Stoney's letter which is not erroneous. He says:—"I cannot find anywhere in Prof. Osborne Reynolds's writings an explanation of the thing to be explained, viz., that the stress in a Crookes's layer is different in one direction from what it is at right angles to that direction."

I do not at all admit that this is "the thing to be explained," and I am quite sure that Mr. Stoney would find no explanation of it in my writings.

In the passage quoted above Mr. Stoney has, for the first time, so far as I know, expressly stated his belief that Mr. Crookes's phenomena depend on such a difference of stress. I have thought all along that his views were based on such an assumption, but I did not like to take it for granted. It is almost a pity, if I may use the phrase, that he did not express himself thus clearly at first, as in that case I might have done before what I am about to do now, viz., prove definitely that such a condition of stress can have nothing to do with the cause of Mr. Crookes's results—that, so far from explaining, such a condition of stress is inconsistent with, these results, and this, not in mere matters of detail, but as regards the fundamental direction in which the force acts.

Throughout all the experiments that have been made one invariable law as to the direction of motion has been found to maintain, which is that the force always tends to drive the vanes or bodies in the direction of their colder faces. Thus when a body is free to move in a sufficiently rarefied medium, if its front be heated it will move backward, while if its front be cooled it will move forward, always moving towards its colder face. There are no exceptions to this rule.

Let us now suppose that we have two bodies, A and B, free to move in a sufficiently rarefied medium. Suppose A to be initially hot and B cold, while the medium and surrounding surface are at the mean temperature of A and B. Then, owing to the radiation of heat between the two bodies, that side of A which is opposite to B will be cooled faster, and hence be colder than the other side of A. Hence according to the law stated above, A must move towards B, and this it is found to do by experiment. On the other hand, that side of B which is opposite to A will become heated by radiation faster, and hence become hotter, than the other side of B, and hence B will move away from A. Thus if both bodies were free to move, we should have B running away from A, and A running after B.

This aspect of the phenomenon is perhaps the most paradoxical that presents itself; it is nevertheless in strict accordance with experiment, and it was by instancing this case that I was enabled to show that the force could not by any possibility be directly due to radiation (see *Phil. Trans.*, vol. 166, p. 728).

The same reasoning now enables me to show, just as conclusively, that the force which causes the motion in the bodies cannot be due to the stress, in the layer of gas which separates the bodies, being greater in the direction joining the bodies than it is at right angles to this direction. For the only effect of such a difference in the stress would be to cause the bodies to separate; therefore, instead of A following B, it would be forced back in the direction of its hottest side, or in a direction opposite to that in which it is found experimentally to move.

This case, therefore, shows the fundamental error of Mr. Stoney's view. Although he allows that the intervening gas is the medium of communication, he assumes, none the less, that the force acts directly between the two bodies (the heater and cooler), in which case action and reaction must be equal between the two bodies. Experiment, on the other hand, shows conclusively that the force acts independently between each body and the gas which surrounds it; the pressure being always greatest on the hottest side. The force which acts on the body reacts on the gas, causing it to move in the opposite direction, and the wind thus caused tends to carry all opposing obstacles with it. Hence, in the case above, the motion given to the air at the one body must to some extent affect the opposing surface, but this surface forms only one obstacle, while the action of the wind is distributed throughout the entire chamber, in which it acts in the manner so beautifully shown by Dr. Schuster's plan of suspending the vessel. A simple analogy to what happens in the case of A and B is furnished by two steamboats, the one following the other. The water thrown back by the screw of the first would stop the second, but only to a small extent.

When answering Prof. Foster in a former letter, I said "that it is contrary to the kinetic theory that the increase resulting from rarefaction in the mean path of the gaseous molecules should favour the action." In making this statement all I meant to imply was that the action was independent of any relation between the mean path and the distance of the hot surface from the cold surface, which was the only point in question. Although my statement was strictly true in this sense, it appears to me, on further consideration, that it might include more than I intended.

I hope that nothing I have said, either in my earlier papers or in this controversy, has led any one to suppose that I regarded my explanation as entirely complete. I suggested, and to some extent established, the true source of the force, namely the heat communicated to the residual gas, and although now my suggestion appears to have been universally accepted, it may be remembered that at the time my first paper was written the only other suggestions as to the cause of the motion observed by Mr. Crookes were of a widely different character. As regards the working out of the detail of my explanation, there has been one point which I could not quite see through, viz., the influence which the hot molecules receding from the surface might have on the rate at which the cold ones would come up, and although I have been trying to satisfy myself on this point ever since my first paper was published, it is only within the last three months that I succeeded.

Now, however, I have arrived at a result which, although somewhat unexpected and striking, will, I hope, be found to reconcile what has hitherto appeared to be anomalous in the phenomena already known, and to have suggested certain hitherto unexpected phenomena which now only await experimental verification.

OSBORNE REYNOLDS

January 15

Sun-spots and Terrestrial Magnetism

PRECISELY because the article (NATURE, vol. xvii. p. 183) on "The Sun's Magnetic Action at the Present Time," is by so able a mathematical physicist as Mr. John Allan Broun, and because of all sides of the solar problem there is none wherein he is so *facile princeps* as the magnetic, I venture to think this a good opportunity for asking a question which has troubled me much of late, and which is this:—

The sun-spot cycle and the terrestrial magnetic diurnal oscillation cycle are looked on now generally as being, if not actual cause and effect, at least as equally both of them effects of one and the same cause, and necessarily, therefore, synchronous. Yet if we inquire of the sun-spot observers the length of their cycle, they declare it (as see Prof. Rudolph Wolf's admirable

and exhaustive paper in the last volume of the *Memoirs* of the Royal Astronomical Society) to be 11·111 years. While if we ask the magnetic men the length of the cycle of their needle manifestations, they (as in Mr. Allan Broun's first paragraph on p. 183) declare it as confidently to be 10·5 years.

Wherefore I would request to be kindly informed if the maxima of the two cycles do approximately agree just now, where will they be, relatively to each other, after a dozen cycles hence? And the answer may or may not assist in clearing up certain apparent anomalies in the Edinburgh earth-thermometer observations. PIAZZI SMYTH

15, Royal Terrace, Edinburgh, January 11

On the Insects of Chili and New Zealand

IN Mr. McLachlan's note "On Some Peculiar Points in the Insect Fauna of Chili" (NATURE, vol. xvii. p. 162), I see, with surprise, the remark that "the large islands of New Zealand furnish us with no indication whatever of forms parallel with those found in Chili," for it is well known that many Lepidoptera belonging to European genera do occur in New Zealand, although, perhaps, neither *Argynnis* or *Colias*. Amongst a small number of Lepidoptera from New Zealand which lately came into my hands, I notice species of the following European genera:—*Sesia, Cloantha, Nonagria, Heliothis, Hybernia, Larentia, Fidonia, Cidaria, Corenia, Camptogramma, Asthena, Acidalia, Scoparia*. Except in the case of *Sesia tipuliformis*, it is not probable man has had any hand in the introduction of them. None, except the *Sesia*, are identical with European species, although several approximate, and the causes which have led to the existence of *Agynnis* and *Colias*, in Chili, are probably the same as those which have planted the insects I have named in New Zealand.

In Mr. Darwin's "Origin of Species," Chapter XII., we find a suggested Explanation of the Presence of the Forms of the Northern Temperate Zone in South America and New Zealand in the occurrence of alternate glacial epochs at the North and South Poles, and although the observations especially refer to plants, they are applicable to the insects which would, doubtless, accompany them in their supposed migrations. Perhaps it is not an entirely satisfactory explanation, and with his usual candour, Mr. Darwin admits that it does not meet all difficulties. In describing the wanderings of the plants, Mr. Darwin uses terms (figurative of course) which endow them with extraordinary if not voluntary powers of locomotion, as, indeed, they would seem to require in reality, for effecting such wonderful migrations, and as regards insects Mr. McLachlan goes further, and suggests that some of them "mistook the points of the compass and went southward."

Now the pertinacity with which the Lepidoptera adhere to particular plants and stations, and prefer death to change of either, is a much more noticeable character than their ability to emigrate, and seems to me a serious bar to the acceptance of a theory involving great changes of food and a double journey across the equator; possibly some of the polyphagous species might survive it, but even these, according to Mr. McLachlan, appear to have got a little muddled in their reckoning. Most of the insects I have named are eminently select in their diet, and how are we even to conceive of the wingless female of Hybernia performing the vast journey?

I do not know that we have evidence that change of climate induces migration of the Lepidoptera. There is a large colony of *Bryophila perla*, which has been stationed on an old wall here for the last twenty years, and although there are miles of similar lichen-covered walls in the neighbourhood, I have never seen a specimen fifty yards from head-quarters, and even under the threat of a new glacial epoch, I do not think it would consent to move on.

In saying there are no indications of similar forms on the northern portions of the Andes, I am not sure whether Mr. McLachlan refers to Lepidoptera or Trichoptera, so I will mention that I have received several species of *Colias* captured on the eastern Cordillera of New Granada. The genus probably ranges through the whole chain of the Andes.

Douglas, Isle of Man, January 2 EDWIN BIRCHALL

Macrosilia cluentius

IN NATURE (vol. viii. p. 223) I have spoken of a *Sphinx* which, with its proboscis of 0·25 metre length, would be capable of obtaining nearly all the nectar of *Anagræcum sesquipedale*. Lately my brother, Fritz Müller (Itajahy, Prov. St. Catharina, Brazil), sent me the wings of another specimen of the same species, and Dr. Staudinger, of Dresden, stated by comparison of these wings with the Sphingidæ of his collection that the name of the species is *Macrosilia cluentius*, Cramer.

Lippstadt, January 9 HERMANN MÜLLER

Meteor

I TAKE the liberty of forwarding the following particulars relative to a meteor which I saw on Sunday last at 4h. 24m. P.M., that is to say, about twenty minutes after sunset. As, however, the day had been very fine, there was not only full daylight in the west, but only a trace of twilight in the north-west direction, in which I saw the meteor. I may add that the sky was slightly overcast by watery clouds in that direction:—

Point from which seen, Salthill, near Kingstown; direction in which seen, north-west; elevation above horizon, 10° to 15°; length of luminous "tail," 5° to 6°; inclination from vertical, about (towards south) 10°; time, 4h. 24m. P.M.; colour of tail and of globe of explosion, light blue.

Judging from the elevation and from the fact of its being visible notwithstanding the strong twilight and the interposed clouds, I conclude that this meteor must have been remarkably brilliant and that it exploded over or beyond the West Coast of Ireland. It is for these reasons that I take the liberty of calling attention to it, as others may have seen it under more favourable conditions. P. W. REILLY

Royal College of Science for Ireland,
Stephen's Green, Dublin, January 15

Philadelphia Diplomas

IN NATURE, vol. xvii. p. 183, there appears a note by Dr. C. M. Ingleby on the "Philadelphia Diplomas." Permit me to say that the only institutions in Philadelphia legally authorised to grant medical diplomas are the University of Pennsylvania, a school which has long ago celebrated its centenary, and the Jefferson Medical College. The so-called University of Philadelphia is a hybrid concern, the medical department of which is under the management of the Eclectic Medical School.

January 10 RICHD. C. BRANDEIS

Great Waterfalls

I SHALL be much obliged if you, or any of your readers, can inform me in what book I can find accounts of any of the following great waterfalls:—The Tequendama Fall, near Sta. Fé de Bogota, South America; the Cauvery Falls, near Seringapatam, India; the Alatau Falls, Alatau Mountains, Central Asia; the Guava, or Guayra Falls, on the Alto Parana, South Brazil; Falls of the Rio Grande, near Guadalajara, Mexico. These great falls, five of the most remarkable in the world, are shortly noticed in books of geography, but I have hitherto been unable to obtain any detailed particulars or description of them.

Eltham, January 7 ARTHUR G. GUILLEMARD

BIOLOGICAL NOTES

SELF-FERTILISATION OF PLANTS.—This subject, around which the genius of Mr. Charles Darwin has thrown a halo, seems likely to give rise to further controversy. The Rev. G. Henslow, in a communication laid before the first meeting this session of the Linnean Society, gave an exposition of the views he had arrived at; these in many respects being at variance with those promulgated by Mr. Darwin. The author acknowledged how indebted he stood towards the latter, whose vast storehouse of facts and close reasoning necessitated constant reference to his writings; but the author's own deductions therefrom, and additional researches, nevertheless, confirmed him in hesitating to accept some of Mr. Darwin's conclusions. According to Mr. Henslow, the chief facts and bearings of the self-fertilisation of plants may thus be summarised: 1. The majority of flowering plants are self-fertile. 2. Very few are known to be physiologically self-sterile. 3. Many are morphologically self-sterile. 4. Self-sterile plants become self-fertile by (*a*) withering of